regional drift may remain uneliminated, so that it is not possible to apply a map of India to a map of the whole world with certainty. As this doubt has been authoritatively expressed, it seems worth while to point out that lunar observation would detect such drift, whether in latitude or longitude. In the case of latitude, that found from the Moon would differ by 1″ from that found from stars, and in the case of longitude that found from occultations would differ 2^s (30″) from that found from star signals, if $d\phi$ or $d\lambda$ attain to 1′.

Or, better, on the same night, longitudes found from occultations at large hour-angles would differ nearly 2^s (30″) from those observed near the meridian.

Occultations appear to be very suitable, but as long as they are corrected for ephemeris errors obtained with transit instruments, they are no better than transit observations. In the case when two occultations are observed on one night, or one in two localities, this objection is removed.

It may be added that a marked discrepancy was found in the fundamental Indian longitude based on a long series of Moon culminations, and the same later determined telegraphically, and this note may help to explain it.

Abbasia Observatory, Egypt: 1903 *October* 26.

Remarks on a Paper by Mr. Cooke on a New Method of Determining Time, Latitude, and Azimuth. By E. B. H. Wade, M.A.

(*Communicated by Professor H. H. Turner.*)

At the beginning of the present year Mr. Cooke described a new method of determining time, latitude, and azimuth (*Monthly Notices, R.A.S.*, vol. lxiii. p. 156). As I have been employing precisely his method since May 1902, I thought it would be permissible to make a few additional remarks on the subject.

Having been instructed to determine the latitude of Tamia in the Fayum Province of Egypt, I proceeded thither with an 8-inch theodolite, intending to rely on circle readings. However, a very strong wind was blowing, and this caused such inconvenience in illuminating graduations, that Talcott's method seemed preferable. For this a vertical micrometer is necessary, and the one furnished with my instrument could not be turned into the vertical without structural alterations. I therefore decided to make my chronometer a substitute for the micrometer. For example, if the second of a pair of stars pass the meridian at a greater altitude than the first, then by placing the instrument out of the meridian for the second star we may still secure it at the same altitude as the first. It at once became evident that

pairs of stars could thus be utilised which would not come within the limited range of Talcott's method. Moreover, it was now necessary to know the error of the chronometer, and for this purpose I included stars in the neighbourhood of the prime vertical. Thus, by degrees, I had come to employ as a substitute for Talcott's method, one differing in no respect from that described by Mr. Cooke.

As the results were satisfactory, I contemplated publishing them, but a study of the literature of the subject led me to think that there was little if anything original in the method. I therefore contented myself with writing a departmental minute on the subject, and inserting an appendix on it in the annual report of the Observatory for 1902. In the first place the method differed only from Chandler's* in the use of a spirit level; again, Chauvenet had described a method for finding latitude and time by the observation of three or more stars at equal altitudes.† At the time when he wrote, Chandler's device had not been invented, and the instrument which he had in mind must have been a sextant, a zenith telescope, or a theodolite. Even if he had not a theodolite in mind, the use of a theodolite as zenith telescope did not appear to me new. For instance, Sir David Gill has used theodolites in place of zenith telescopes on the survey of South Africa.‡ However, the method, even if not strictly new, has probably escaped the notice of surveyors, and it is therefore fortunate that Mr. Cooke has drawn attention to the subject.

Applications.

In my own case I have used the method for latitudes and time only. I deliberately rejected it as a means of obtaining azimuths.

Mr. Cooke refers to the difficulties arising from circle graduations and collimation. To these we may add the reading of a striding level and the pivot errors. All these must be reckoned with to obtain the azimuth of a terrestrial mark. I have preferred a method specially designed to mitigate these evils. An account of it will appear in the report above referred to.

Definitions.

In this note I propose to use the following definitions :—

ϕ = observer's latitude.

θ = sidereal time of observation.

$$\left.\begin{array}{l} h = \text{star's altitude} \\ A = \text{star's azimuth} \end{array}\right\} \text{at time } \theta.$$

δ = star's declination.

* *Annals of Harvard College Observatory*, vol. xvii.
† *Spherical and Practical Astronomy*, vol. i. p. 280.
‡ *Report on the Geodetic Survey of South Africa*, 1894.

Practice.

My practice appears to be the same as Mr. Cooke's in all details except the following :—

1. I usually point the instrument to *Polaris* at the beginning of the set, instead of the altitude ϕ + refraction. But the method of calculation (given below) leaves me quite free in this matter, and the set may be taken at any convenient altitude. If the method is to apply to equatorial latitudes, the constant altitude must evidently differ much from that of the pole.

2. The azimuth circle is never read for the reasons stated above. I would add that if it is to be read, and the observer's object is to find latitude and azimuth only and in one operation, the timekeeper may be dispensed with, which might be a great convenience in the field. Consider a special case. A star of known declination north of the zenith cuts the circle of equal altitude twice, and the horizontal readings are A_1A_2. Another to the south does so at readings A_3A_4. Either $\frac{A_1+A_2}{2}$ or $\frac{A_3+A_4}{2}$ is the meridional reading of the theodolite, and the relation between A_1-A_2 and A_3-A_4 leads to the latitude. A general theory for any number of stars is easily worked out.

Computation.

From the fact that we have been working independently, my method of computation is totally different from Mr. Cooke's. I begin by assuming a probable value for the latitude of the place and the error of my chronometer. I then calculate on these assumptions the zenith distance of every star in the set from the formula,

$$\sin h = \sin \phi \sin \delta + \cos \phi \cos \delta \cos t.$$

I make no attempt, as does Mr. Cooke, to group my stars into pairs. All stars are calculated in exactly the same way. Now, supposing that the assumption as to time and latitude is correct, and further that the "bubble" readings are throughout the same, and that the error of observation is negligible, then all these computed zenith distances will work out exactly equal ; but, if not, each star will give us an equation of the form,

$$h + \Phi d\phi + \mathrm{T}dt - h_0 = 0.$$

In these equations the quantities Φ and T are the differential coefficients expressing the result of errors of chronometer or latitude upon the calculated zenith distance. These coefficients may be obtained by the ordinary rules of spherical trigonometry, and the unknown quantities, $d\phi$, dt, h_0, may then be found by the method of least squares. In my earliest observations I

proceeded thus, but in later ones I have employed the modification about to be described.

In the table below the first column is the ordinary calculation of an altitude. The second shows the changes (in units of the seventh place) produced by increasing ϕ by 10″; these are found at once from the proportional parts 44 and −10 for sin ϕ and cos ϕ respectively. Similarly, the third column shows the effect of an increase in t by 10″; it is found from the proportional parts for cos t; namely, 104.

	I.	II.	III.
log sin ϕ	1. 6377 167	44	...
log sin δ	1. 8075 742	...	...
sum	1. 4452 909	44	...
nat (1)	0. 2757 991	29	...
log cos ϕ	1. 9546 301	−10	...
log cos δ	1. 8846 006	...	...
log cos t	1. 2958 659	...	−104
sum	0. 1350 966	−10	−104
nat (2)	0. 1364 887	− 3	− 33
(1)+(2)	0. 4152 878	26	− 33
log (1+2)	0. 6153 492	27	− 35
$h=$	24° 32′ 14″, 8	6″, 0	−7″, 5

The equation for the star is therefore

$$0{\cdot}60\, d\phi - 0{\cdot}75\, dt + 14''{\cdot}8 - h_0 = 0$$

Results.

I proceed to give some results with the method. I would first mention that they are, strictly speaking, field results. Without wishing to be unduly critical, I question whether Mr. Cooke's results were obtained at such a distance from his observatory that they can really be compared with field results obtained by other observers. Working at hundreds of miles from the nearest centre of civilisation, we are beset with petty difficulties which tend to diminish accuracy. The further, however, that we go into the field, the more we are struck by the suitability of the present method for field purposes.

1. *Station Tamia, June* 21, 1902.

Latitude found 29° 28′ 0″ − 4″, 65. $\Delta\theta = +46''\ 2.$

Residuals given by stars

	″
β Cassiopeiæ	2·1
α Cassiopeiæ	2·4

δ Capricorni	3″·6	
γ Capricorni	1·0	East of meridian.
β Capricorni	−1·7	
γ Capricorni	−4·3	West of meridian.
δ Cassiopeiæ	−2·2	
ι Herculis	−2·2	

This example is given chiefly on account of its early date.

2. *Station near Luxor.*

	Latitude found.	Δθ.
	° ′ ″	″
1903 Feb. 26	25 44 12, 0	+11, 0
27	10, 4	+0, 0
28	11, 0	+12, 0

In this set as the result of increased experience the residuals are very much smaller than at Tamia. On the last two nights no single residual exceeded 1″·5.

The observations near Luxor are more suitable than those at Tamia for a discussion of accuracy. In the first place, there are three nights of observation; and secondly, the result can be compared with an independent method. Owing to the clearness of the sky in Egypt it was found possible to observe the altitude of the pole star in broad daylight, reading the circles of the instrument with micrometer microscopes under the most favourable conditions as to illumination. The arc of the instrument employed in this set was subsequently examined at the Observatory, and the following latitudes were obtained by the ordinary methods. For convenience the results obtained by the two methods are placed side by side:

	By Polaris.	By Method.		By Polaris.	By Method.
	° ′ ″	″		° ′ ″	″
Feb. 25	25 44 10·7		Feb. 26	25 44 11·4	
	12·2			10·8	
	11·0	...			
	11·0		27	12·0	
				11·3	10·4
26	9·9			12·5	
	10·8				
	8·4	12·0	28	—	11·0
	9·8				

Mean by Polaris.	Mean by Method.
25° 44′ 10″·9	25° 44′ 11″·1

Apparently the accuracy which I have reached is rather less than Mr. Cooke's, but my results, I think, confirm his opinion as to the convenience of the method. I may add that the times which I have obtained by this method have only been needed for approximate purposes, but I believe them to be quite accurate. In May of the present year I received instructions to determine the latitude and longitude of Tema, and made my arrangements to obtain both latitude and accurate local time by the method; but the telegraphic arrangements fell through, so that the method was only applied to the determination of latitude. I am convinced, however, that more accurate local times can be obtained in this way than by any other field method.

Relative Accuracy of Mr. Cooke's and other Methods.

It is interesting to inquire to what circumstance this method owes its supposed superiority over other field methods, including Talcott's. Given a faultless micrometer, Talcott's is a special case of Mr. Cooke's, and the one in which the error in zenith distance has the minimum effect on the latitude. If the superiority is confirmed, it would seem to imply that the accuracy of Talcott's method has hitherto been limited by micrometer errors.

My best thanks are due to Professor Turner for encouragement in this investigation, to the Survey Department of Egypt for providing facilities for the observations, and to the Under Secretary of State for Egyptian Public Works for permission to publish this note.

Abbasia Observatory, Egypt:
1903 *October.*

On the use of the Stereo-comparator for Plates on which a Réseau has been Impressed. By Dr. Max Wolf.

[*Introductory Note by Professor H. H. Turner.*—The accounts given of the ease and rapidity with which two plates of the same region can be compared with the stereo-comparator naturally create a desire to possess such an instrument. A generous offer has been made to present one to the Oxford University Observatory, *provided that it will clearly be of use in our work.* The very proper condition specified at once raises the question whether plates which have a réseau impressed upon them, and which have several images of the same star, can be scanned as readily by the stereoscopic method as those containing only a single image of each star and no réseau; for all our plates accumulated hitherto at Oxford are of the former class. For some time I hoped to be able to obtain a stereo-comparator on loan to make